

# Parapsychology: Science of the anomalous or search for the soul?

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**Abstract:** Although there has been over a century of formal empirical inquiry, parapsychologists have clearly failed to produce a single reliable demonstration of "paranormal," or "psi," phenomena. Although many parapsychological research projects have been carried out under what have been described as well-controlled conditions, this does not by itself make a science, for unless and until it can be demonstrated that paranormal phenomena really exist, there is no subject matter around which a science can develop. Indeed, parapsychologists have not even succeeded in developing a reasonable definition of paranormal phenomena that does not involve, or imply, some aspect of mind-body dualism. Moreover, parapsychology has developed several principles (such as the experimenter effect) which can be used to explain away failures, and the use of these principles contributes to making the psi-hypothesis unfalsifiable.

The "anything goes" attitude in parapsychology, which seems to lend credence to virtually any "paranormal" claim, serves to weaken the credibility of parapsychological endeavors in the eyes of critics. This general willingness to suspend doubt is another indication that parapsychology is more than the quest to explain anomalous experiences, as is claimed. It is argued in this paper that parapsychological inquiry reflects the attempt to establish the reality of a nonmaterial aspect of human existence, rather than a search for explanations for anomalous phenomena.

**Keywords:** anomaly; causality; dualism; ESP; experimental method; explanation; methodology; parapsychology; philosophy of science; replication

## 0. Introduction

It is curious that, in this age of unprecedented literacy and unceasing scientific and technological progress, many people are prepared to accept that spoons can be bent by the power of the mind alone, that disease can be cured by the laying on of hands, that water can be located by means of a forked willow stick, or that the mind can influence the decay of radioactive substances. It is even more curious when such claims are put forth and defended by people trained in the ways of science.

Most *BBS* readers, I would imagine, have little difficulty dismissing popular occult beliefs in astrology, palmistry, the tarot, or biorhythms. However, those same readers may not be nearly so cavalier about disregarding such supposed "paranormal" (also synonymously referred to as "parapsychological" or "psi") phenomena as extrasensory perception ("ESP") or psychokinesis ("PK"). ESP refers to the supposed ability to obtain knowledge of a target object or of another person's mental activity in the absence of sensory contact, and PK is the putative ability of the mind to influence matter directly. Belief in such phenomena is actually very widespread, not only among members of the general public but also among university students (e.g., Alcock 1981; Gray 1984; Otis & Alcock 1982).

Such belief is no doubt tied, at least in part, to the fact that many people, perhaps even most, have from time to time had direct personal experiences that seemed to be "telepathic" or "precognitive" or "psychokinetic." Indeed, a number of surveys (e.g., Alcock 1981; Evans

1973; Irwin 1985a; McConnell 1977; Sheils & Berg 1977) have found personal experience to be the major reason given by respondents for their belief in paranormal phenomena. This is not surprising: Given their often powerful emotional impact, combined with a lack of understanding about the myriad "normal" ways in which these experiences can come about (e.g., see Alcock 1981; Marks & Kammann 1980; Neher 1980; Reed 1972; Zusne & Jones 1982), it is easy to ascribe paranormal explanations to odd experiences that one cannot readily explain otherwise.

"Parapsychology" is defined as the scientific study of paranormal phenomena (Thalbourne 1982). The study of the paranormal was historically associated with the so-called occult sciences such as astrology and numerology; a more direct progenitor was the spiritualism craze of the late nineteenth and early twentieth centuries. However, parapsychology stands well apart from these belief systems in a number of ways:

**0.1. Scientific orientation.** For over a century, there has been careful and deliberate investigation of psi phenomena by people trained in the methods of science. In the past 50 years, much of this research has been laboratory-based and carried out in university settings. Currently, parapsychological research is being conducted at such prestigious academic institutions as the University of Edinburgh and Princeton University.

Throughout the last century and continuing to the present, a number of very prominent natural and social scientists have been proponents and supporters of para-

psychological research (see Hyman 1985a; Rogo 1986), including physicists Sir William Crookes, Lord Rayleigh (Nobel Prize, 1904), Wolfgang Pauli (Nobel Prize, 1945), Brian Josephson (Nobel Prize, 1973), and David Bohm; naturalist Alfred Russell Wallace; chemist Robert Hare; physiologist Charles Richet (Nobel Prize, 1913); psychologists William James, William McDougall, Carl Jung, Sir Cyril Burt, and Hans Eysenck; anthropologist Margaret Mead; mathematician John Taylor (who became convinced of the reality of psi phenomena on the basis of Uri Geller's purported feats [Taylor 1975], only subsequently to repudiate his belief in such phenomena [Taylor & Balanovski 1979]); and Robert Jahn of the Engineering Department at Princeton University.

There has also been a history of professional interaction between conventional science and parapsychology at scientific conferences, through symposia on the paranormal and invited addresses by parapsychologists (e.g., American Association for the Advancement of Science, 1975, 1978, 1984; American Physical Society, 1979; American Psychological Association, 1966, 1967, 1975, 1984, 1985), although admittedly such opportunities for parapsychologists to present their ideas and evidence have been limited.

**0.2. Organization.** As a research discipline, parapsychology is organized very much the way various disciplines of mainstream science are. There are professional bodies that emphasize empirical enquiry using scientific methodology and that encourage high research standards. (One of these, the Parapsychological Association, over half of whose 300 or so members hold doctorates in science, engineering, or medicine [McConnell 1983], has been affiliated with the American Association for the Advancement of Science since 1969.) Annual research conferences are held. Research grants are awarded. There is substantial empirical literature in the field – including several research journals and many books, some of which have been published by leading scientific publishers (e.g., the *Handbook of parapsychology* [Wolman 1977a], the *Foundations of parapsychology* [Edge et al. 1986], and the series *Advances in parapsychological research* [Krippner 1977; 1978a; 1982a; 1984].)

**0.3. Academic involvement.** Courses in parapsychology are offered for academic credit at about 50 colleges and universities in the United States (McConnell 1983); a few even grant degrees in the subject (see Stanford 1978). Ph.D.'s have been awarded for parapsychological research at Cambridge University, the University of Edinburgh, Surrey University, Purdue University, the University of the Witwatersrand, and the City University of New York, among others. The University of Edinburgh has recently established the Koestler Chair in Parapsychology, which is endowed by a bequest from the late Arthur Koestler, a long-time supporter of parapsychology.

How do members of academia view claims about psi? In one survey of humanities and science professors at two large universities (University of Michigan and University of Toronto; response rate 53%), only about one-third of the respondents indicated believing in paranormal phe-

nomena (Otis & Alcock 1982); there was no clear difference between representatives of the sciences and the humanities. This is consistent with the results of a smaller survey conducted at two other Canadian universities (Alcock 1981). Yet, Wagner and Monnet (1979), in a much larger survey of professors at 120 colleges and universities in the United States (response rate 49.5%), found that 73% of the respondents from the humanities, arts, and education indicated they believed ESP to be either an established fact or a likely possibility, whereas only 55% of the respondents from the natural sciences and 34% of the psychologists did likewise. Whether the differences between the results of the two surveys reflect differences in the questions asked or differences in the groups sampled (the former study was limited to respondents from two large and prestigious universities) is not clear. (It should be noted in any case that such surveys are always subject to a response bias, in that there is likely to be a differential response rate as a function of attitude toward the subject matter being addressed.)

Although all of this might suggest that parapsychology is a serious and professional research discipline that is viewed with respect within university settings, at best parapsychology struggles to maintain a toe-hold at the fringes of academia; mainstream science continues virtually to ignore its subject matter or even to reject and ridicule it. One finds no mention of psi phenomena in textbooks of physics or chemistry or biology. Lecturers do not address the paranormal in undergraduate or graduate science programs. Psychology students are rarely taught anything about the subject. Parapsychological research papers are only very infrequently published in the journals of "normal" science, and parapsychologists have criticized leading scientific publications such as *Science*, *The American Journal of Physics*, and *American Psychologist* for suppressing the dissemination of parapsychological research findings (Honorton 1978a; McConnell 1983). Funds for parapsychological research are usually generated within parapsychology itself or come from private donors; the agencies that fund normal science turn a blind, or even hostile, eye toward parapsychological research proposals. The United States government, however, has provided multi-million-dollar support for psi research into remote viewing at SRI International in California [Targ & Harary 1984].)

What accounts for the disparity between what would seem to be a substantial degree of professionalism in parapsychology on the one hand, and the continuing relegation of parapsychology to the fringes of science on the other? For one thing, parapsychology continually encounters opposition from mainstream psychology; psychologists appear to constitute the most skeptical group concerning whether psi is likely to exist (Alcock 1981; Wagner & Monnet 1979). Second, people who may serve as the "gatekeepers" of science, in that they are very influential in determining what is and is not the proper subject matter of science, are skeptical about psi. A recent survey of "elite" scientists (Council members and selected section committee members of the American Association for the Advancement of Science) revealed the highest level of skepticism regarding ESP of any group surveyed in the last 20 years (McClennon 1982): Fewer than 4% of the 339 respondents (the response rate was

viewed ESP as scientifically established. (However, 25% considered it to be a likely possibility, cating about the same proportion of favourableness as reported by Otis and Alcock [1982], cited above.) Fifty ent considered ESP to be impossible or a remote sibility.

In McClenon's (1982) view, this negativity is based on threat that paranormal phenomena, were they to st, would pose to the prevailing scientific worldview. A ter different viewpoint, which is part of the thesis of paper, is that parapsychology, over its century or so of stence as an empirical research endeavor, has simply ed to produce evidence worthy of scientific status. Of use, *both* these views could be correct.

To facilitate the discussion of this issue, I shall proceed posing a number of questions I consider to be import- it concerning psi and parapsychology:

1. What is psi; how is it defined?
2. Is psi "possible"?
3. If psi exists, how can it be detected?
4. What is the evidence that psi exists?
5. Does parapsychology follow the rules of science?
6. Are the critics fair?
7. Is rapprochement possible between psychology and parapsychology?

Let us consider each of these questions in turn.

### **. What is psi?**

lthough it may at first seem straightforward to define or atalogue paranormal phenomena, it turns out to be a ifficult task indeed, for there is a considerable spectrum f opinion even within parapsychology as to which ostensible phenomena are likely to be genuinely paranormal nd which are probably based on error and self-delusion. For example, although many parapsychologists might scoff at such claims, some believe that "psychic healers," hrough the laying on of hands, can speed the healing of wounds and slow the growth of fungi (Krippner 1982b); others believe that some gifted persons can project images onto photographic film (Eisenbud 1977), that water ources, or even lost treasure, can be located by "dowsing" with a willow stick (Bird 1977; Schmeidler 1977), that reincarnation warrants serious investigation (Child 1984; Stevenson 1977), that one's personality can leave and return to the body at will and may even be able to travel through outer space (Targ & Puthoff 1977), and that deathbed visions may be indicative of survival after death (Otis & Haraldsson 1978).

Because there is no general agreement on what psi is, or at least how it manifests itself, parapsychologists have found it easier to define it in terms of what it is *not*. The term "psi" itself was introduced by Thouless (1942) as a neutral label in order to avoid the many associations that terms such as "psychic phenomena" and "extrasensory perception" have developed over the years, and psi is defined simply as "interactions between organisms and their environment (including other organisms) which are *not* mediated by recognized sensorimotor functions" (Krippner 1977, p. 2; my italics). Psi phenomena, then, are explicitly defined in a negative manner: To demonstrate that psi has occurred, one must first eliminate all *normal* sensorimotor explanations.

Although only a few parapsychologists appear to share his conservatism, Palmer (1985a; 1986a) argues that until parapsychologists have produced a positive theory of psi which describes the properties that must be present in order to claim that psi has occurred, all they can claim to have demonstrated is the occurrence of a number of anomalies which themselves constitute the subject matter of psi. Seemingly paranormal events might be explicable in terms of conventional science or science as it will be understood in the future, he says, or, indeed, such events might be due to errors in interpretation or measurement or statistical analysis. He recommends that the term "paranormal phenomena" be supplanted by a much less committed term such as "ostensible psychic events."

Palmer's circumspection is commendable and would find favour with most critics of parapsychology. However, it is rare to find parapsychological research reports or other kinds of literature treating apparent anomalies in such a noncommittal fashion. Most, in fact, treat psi not as a description of an anomaly but as a causative agent.

There is a second and more important sense in which psi is negatively defined, albeit implicitly, and that is in terms of its incompatibility with the prevailing scientific worldview (Boring 1966; Flew 1980; Mackenzie & Mackenzie 1980): In some way or another, psi phenomena, to be considered as such, are impossible if the current worldview is correct. There are two different camps within modern parapsychology regarding this incom- patibility (Beloff 1977):

**1.1. Incompleteness of current science.** Just as the scientific worldview changed to accept the extraterrestrial source of meteorites and the constancy of the speed of light, so too, according to this viewpoint, it must ultimately accommodate psi. Thus, "paranormal" phenomena are part of the natural order, but a part of that order which is not yet understood; as soon as scientific knowledge advances to the point that the paranormal is comprehensible, then the latter will become part of an expanded normal science (Truzzi 1982).

This process has been manifested already in several instances: Bat navigation was taken to involve psi until the echo-sounding apparatus of bats was discovered, at which time it became part of the normal scientific domain and of no further interest to parapsychologists (Boring 1966). Bird navigation (Pratt 1953; 1956) and hypnosis (see McConnell 1983; Spanos 1986) are other examples of phenomena that have passed from the realm of the paranormal to the normal.

**1.2. A nonphysical dimension of existence.** According to this perspective, paranormal phenomena mark the outer limits of the scientific worldview, and beyond those limits "lies the domain of mind liberated from its dependence on the brain. On this view, parapsychology, using the methods of science, becomes a vindication of the essentially spiritual nature of man which must forever defy strict scientific analysis" (Beloff 1977, p. 21).

Of these two perspectives, the incompleteness approach would no doubt be more acceptable to most scientists. Yet, it does not really capture the *flavour* of the paranormal. Whereas anomaly is, it would seem, a necessary condition for par anomality, it is not a sufficient one.

Were it sufficient, then all anomalies throughout the history of science would have to have been considered "paranormal," whereas it is clear that they have not been considered as such (Braude 1978).

Braude (1978) suggests that a definition of the paranormal must go beyond anomaly to include the notion that it "thwarts our familiar expectations about what sorts of things can happen to the sorts of objects involved" (p. 241). Yet, as Mabbett (1982) points out in response to Braude, experimental parapsychological studies that are taken to demonstrate the reality of psi typically produce scoring rates that are only slightly above chance; these hardly thwart peoples' expectations, and "even the thoughtful layman would be unwilling to regard such results as evidence of anything but luck without a little assurance or instruction from the expert statistician" (p. 340).

On the other hand, the bizarre and paradoxical properties of light, as described by relativity theory, would no doubt have been unexpected by laymen as well as by scientists prior to Einstein, Mabbett says, yet most people would not have regarded these properties as paranormal. Mabbett argues that paranormal phenomena are psychological in the sense that they involve mind or consciousness, whatever these may be, and that they reflect a relationship between the mental and physical worlds that is radically different from that conceived of by science.

What is being struggled with here by Braude and Mabbett is that, more than being simple anomalies, paranormal phenomena have a special and particular relationship to the human mind. Indeed, as I have discussed in greater detail elsewhere (Alcock 1985), it is hard to escape the conclusion that the concept of paranormality implicitly involves *mind-body dualism* (see Wolman 1977b), the idea that mental processes cannot be reduced to physical processes and that the mind, or part of it, is nonphysical in nature.

The late Gardner Murphy (1961), once president of the American Psychological Association and one of parapsychology's most erudite and persuasive proponents, argued that even if the paranormal were to be defined only in terms of anomaly, this would still lead to a dualism of some sort because of its independence from considerations of time and space. Indeed, parapsychologists have at times insisted that psi phenomena are distinguished from the other phenomena of psychology by virtue of the fact that they are of a nonphysical nature (e.g., Rhine, J. B. & Pratt 1957). Although the boldness of such a declaration might well raise the hackles of some modern parapsychologists, most of them do seem to accept such dualism (Thalbourne 1984). The influence of dualistic thinking creates a deep schism between parapsychology and modern science.

In summary, then, although some modern parapsychologists prefer to speak only of anomalies, these anomalies, if they are to be of continuing interest to parapsychology, must ultimately involve some radically different relationship between consciousness and the physical world than that held to be possible by contemporary science. Some parapsychologists might deny being mind-body dualists, but they would do well to consider just how they are going to define their subject matter

without some reference to the independence of the mind from the materialistic realm (Rhine, L. E. 1967).

## 2. Is psi "possible"?

Psi phenomena are defined implicitly in terms of their incompatibility with the contemporary scientific world-view. Although many parapsychologists (e.g., Rao 1983) believe that only a major revolution in scientific thought could lead to the accommodation of psi, there have been attempts to reconcile such phenomena with modern science. For example, although it would seem that psi cannot occur without violating well-tested laws of physics — such as the law of conservation of matter and energy and the inverse square law of energy propagation (signal strength is proportional to the inverse square of the distance) — or violating the logical principle that an effect cannot precede its cause, *ad hoc* explanations of how psi might occur without such violation have been proposed (Collins & Pinch 1982). As an example, with regard to the presumed impossibility of seeing into the future, one could posit that what appears to be precognition is really psychokinesis: The individual uses PK to *bring about* the events he believes have been foreseen precognitively. In a similar fashion, one may be able to construct other *ad hoc* explanations to overcome all the various incompatibilities that appear to exist between physical science and parapsychology, although such contrived mechanisms are not likely to satisfy most scientists.

A more direct attempt to render psi compatible with contemporary science has been made through efforts to show that such phenomena are *not* inconsistent with quantum mechanics. In recent years, there has been considerable discussion in parapsychology, led by parapsychologists (parapsychologically oriented physicists) and philosophers, about some of the paradoxes of quantum mechanics and about how it is possible to suggest solutions to these paradoxes that imply the direct influence of the mind on matter, allowing for — or even *demanding* — psi (e.g., Oteri 1975; Schmidt 1975; Walker 1974; 1975).

This has generated negative reaction even within parapsychology (e.g., Braude 1979a), with some parapsychologists such as Phillips (1979; 1984) arguing that the orthodox view of quantum mechanics does *not* lead to paradoxes that necessitate the introduction of mental influences. Phillips describes the difficulty and the arbitrariness of interpreting the mathematical picture served up by quantum theory: "The predictions of quantum mechanics have been verified, and there is little doubt that the mathematical formalism is correct. Constructing a physical picture to correspond to the mathematics is much more difficult, and authors differ in what they find intuitively appealing and philosophically satisfactory" (1984, p. 298).

Even if quantum mechanics did allow for psi — a notion few mainstream scientists would be likely to accept at present — that would not in itself make the reality of psi any more likely. Flying cows are not inconsistent with quantum mechanical notions, but as far as we know, they do not exist. What is missing in such discussions of psi is the phenomenon itself. Until there is clear evidence that psi exists, it is surely premature to try to bend quantum mechanics to accommodate it.

### If psi exists, how can it be detected?

There are three major sources of evidence for psi: (a) anecdotes of spontaneous personal experiences, (b) demonstrations by "gifted" psychics, and (c) laboratory experiments. The early studies of psi examined anecdotal reports in great detail, but gradually the realization grew that such evidence is just too unreliable to serve as data for science (Hövelmann & Krippner 1986; Rhine, L. E. 1977; Rush 1986a).

"Gifted" psychics have provided the most spectacular psi claims, both in the early days of psi research and more recently (Rush 1986b). For some parapsychologists (e.g., Beloff 1985), such demonstrations still stand as strong testimony to the reality of the paranormal. Yet, once again, this evidence is unsatisfactory in the extreme, because of both the history of fraud involving reputedly gifted psychics (e.g., see Girden 1978) and, more important, the fact that such psychics have as yet been unable to perform their feats under controlled conditions for neutral or skeptical investigators. For example, Uri Geller was taken by a number of parapsychologists (e.g., Beloff 1975; Cox 1976; Eisenbud 1976; Hasted 1976; Moss 1976; Puthoff & Targ 1974) to have genuine paranormal powers until a conjurer's investigations (Randi 1975) showed to most people's satisfaction that Geller was using trickery.

Some parapsychologists (e.g., Schmeidler 1984) insist that the fact that a psychic is caught cheating does not weaken the evidential value of those demonstrations during which the same psychic was *not* caught cheating. Given the rarity of such supposedly gifted individuals, it is not surprising that investigators are loath to terminate their research with an individual just because fraud has been detected on some occasions. However, it is no easy task to guard against fraud if a subject is determined to cheat, and what better indication is there of such determination than the subject's being caught at it?

It was because of dissatisfaction with both anecdotal evidence and uncontrolled demonstrations that Joseph Banks Rhine, in the 1930s, set up an experimental laboratory for the study of psi. The hope was that through rigorous application of the methodology of science, psi would soon be put on a solid empirical footing. Rather than simply relying on the ability of self-proclaimed psychics to demonstrate their skills, Rhine began the systematic study of both gifted and ordinary individuals in a number of "guessing" tasks in which probabilities of success could be calculated. If one makes a prediction, based on a probability model, as to how well a subject should score in a guessing task, or if one predicts the distribution of events whose occurrence depends on a random process (in Rhine's day, dice-throwing; nowadays, subatomic particle emission) which the subject attempts to influence mentally, then if all known normal forces have been ruled out, statistically significant departures from the prediction are taken to indicate the involvement of a psi process. Thus, experimental parapsychology, just as conventional psychology had done before it, took on a pronounced statistical flavour.

If one could reliably demonstrate departures from some statistical model, this would call out for explications. There would be no justification, however, for beginning with an explanation based on para-

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psychological concepts. If there were unobserved weaknesses in the controls, if some unknown process were involved (e.g., the use of some code based on silent counting, or the use of "silent" dog whistles that children, but not adults, can hear [Scott & Goldney 1960]), if there were equipment problems or biases in the random generator, if the statistical model were inappropriate, or if errors were made in the recording or analysis of the data, the paranormal explanation would be erroneous. Just as important, in the absence of a positive theory of psi, even if an observed effect is not due to artifact, one is left only with an anomaly. The availability of the psi hypothesis can distract the researcher from other, normal, explanations and thus impede the development of the understanding of anomalies (Blackmore 1983a).

What would constitute "solid" evidence of psi? Obviously, no evidence is ever 100% solid, because we can never be sure how new discoveries will change our understanding of processes that we currently think we understand. Furthermore, evidence that seems unconvincing or unimportant in the light of one theoretical worldview may be viewed as much more important if the prevailing theory changes.

An extraordinary degree of evidence is often demanded in support of extraordinary claims. We are generally less demanding of evidence in the case of claims that "fit" with existing theory or knowledge. When one is weighing evidence in law, the distinction is made between "beyond all reasonable doubt" and "on the balance of probabilities." The former, applied in criminal cases, demands virtual certainty of guilt; the latter, used in civil litigation, refers to the notion that the defendant is more likely than not to be guilty. Because psi is a concept that would probably revolutionize science (Rao 1983), most skeptics implicitly use the criterion of beyond all reasonable doubt, while accepting conclusions made on the balance of probabilities where only "normal" and non-controversial phenomena are involved. However, although the controversial nature of psi may justify the use of tougher criteria, this view has been attacked as being another tactic for denying legitimacy to controversial claims (McClenon 1984).

Before we accept that psi (even in the simplest sense as an anomaly) has been demonstrated in the laboratory, three important factors must be considered:

**3.1. Internal validity.** Psychologists use the term "internal validity" to refer to the degree to which experiments are free of the influence of extraneous variables that might introduce alternative explanations for the observed results (Berkowitz 1986). Most criticisms of experimental studies of psi concern internal validity: Randomization may be inadequate, sensory leakage (i.e., communication of information by normal sensory means) may have occurred, and so forth.

McClenon (1984) argues that such methodological criticisms of psi experiments are often unfair. By refusing to accept the shared assumptions that are implicit in any experiment, he says, the critic will sooner or later "ask for information that is no longer available, or for a degree of experimental control and exactitude that is desirable in principle but impossible in practice" (p. 89). Thus, the "perfect" ESP experiment is an impossibility, McClenon

contends, for one can always suggest that the experimenter was incompetent or that trickery was involved (see also Honorton 1981). Despite McClenon's concerns, there is a considerable difference between making unsubstantiated charges of incompetence or trickery and pointing to methodological flaws. If the flaws are there, parapsychologists should run the experiments again – without the flaws – rather than argue about the motivation of the person who pointed them out.

Rather than rerunning the experiments correctly, a more usual response is to attack the critic. For example, critics have been chastised for pointing to flaws without demonstrating that these flaws are capable of generating the observed departures from chance (Honorton 1975; 1979; Palmer 1986a). This criticism does not stand up, for two reasons. First, critics are usually not advocating the acceptance of an alternate hypothesis but asking only that claims of psi be suspended until properly controlled studies are carried out (Akers 1984; Hyman 1981). Second, such flaws need not be the *cause* of the statistical deviations, but they *are* symptomatic of lax research standards (Hyman 1985b). One should hardly have confidence in the experimental controls if one is faced with evidence of violations of proper procedure. Akers (1984) uses the "dirty test-tube" analogy: A chemist would have little confidence in a colleague's findings if it were observed that a test tube used in the experiment was contaminated.

It is not so difficult to design and execute an experiment that is methodologically and statistically sound. Psychological experiments published in the better psychology journals stand in evidence of this.

**3.2. Consistency.** Before accepting the reality of a phenomenon, one generally looks for signs that there is a consistent pattern of results across experiments. The lack of any consistent pattern in the research findings is one of the most serious weaknesses in the evidence offered for psi (Blackmore 1983a). Unfortunately, it is standard practice in parapsychology to take one pattern of data as evidence for psi in one experiment, then to disregard its absence and take some other pattern as evidence for psi in another experiment.

**3.3. Repeatability.** Not only should there be consistency in the pattern of data across experiments, but individual experiments should be repeatable by others. Repeatability is an important safeguard, albeit only a partial one, against error or fraud (Sommer & Sommer 1984). Obviously, however, replication by itself is not enough. If someone is dishonest in the actual reporting of the research, reports of replication by the same author will not eliminate the dishonesty (Casrud 1984).

Yet, as Rao (1985) points out, repeatability is not a matter of primary concern in normal science. Only if some important and controversial finding is made is replication likely to be attempted, and this will often be undertaken by others who have competing theories that would not accommodate the finding. When observations are consistent with theory, replication is less important. However, as Murphy (1971) commented: "If the event is unclassifiable, then it is doubly important that it have a rational interpretation, that is, one that fits with the thought patterns of the contemporary human mind. If it

has no clear rationality, its only chance of demanding scientific attention is replication" (p. 4).

On this basis, repeatability is, in general, less important in psychology than in parapsychology. Even so, psychologists pay far too little attention to the importance of repeatability (Epstein 1980; Fishman & Neigher 1982; Furchtgott 1984; Heskin 1984; Sommer & Sommer 1983; 1984); replication studies account for a very small percentage (3% or less) in leading psychology journals (Bozarth & Roberts 1972; Sterling 1959). This has led on occasion to the widespread dissemination of information that is subsequently found to be unreplicable (see, for example, Marshall & Zimbardo 1979; Maslach 1979; Schachter & Singer 1979).

Even when replication is attempted, its importance often depends on who conducts it. We are not likely to accept a wild claim supported by the research of only one person, whether that research has been replicated by that person or not (Hyman 1977a). Similarly, a failure to replicate by a student in a high-school science class will carry little or no weight, whereas a failure to replicate by a well-respected scientist will be much more seriously viewed (Collins 1976). It is also difficult to know just what constitutes a replication (Edge & Morris 1986); there are in fact several different kinds of replication that one can provide (Alcock 1981; Lykken 1968). Beloff (1984) differentiates between "weak" and "strong" replicability, where the former term refers to a situation in which an experiment or phenomenon has been independently confirmed by at least one other investigator, and the latter refers to a situation in which any competent researcher, following the prescribed procedure, can obtain the reported effect. Although parapsychologists have presented, as evidence for psi, studies that have been replicated by other parapsychologists, there has never been a psi demonstration that is replicable in the strong sense (Beloff 1973; 1984; Palmer 1985b). Indeed, parapsychologist/psychologist Susan Blackmore (1983a) recently referred to unrepeatability as parapsychology's *only* finding.

Of course, even if a psi experiment is replicated, that by itself does not mean the effect has a paranormal cause. On the other hand, the inability to repeat an experiment or a demonstration cannot by itself rule out the truth of the psi claim. Poor repeatability could conceivably stem from factors other than the nonexistence of psi (Palmer 1986b). It is possible that certain conditions are necessary for the production of psi, and given that no one knows just what these conditions are, it could be that an essential element is missing when an experiment fails to replicate. It has also been suggested that psi could turn out to be inherently unlawful (Palmer 1986b; Rao 1982), although this position is difficult to defend (Hövelmann & Krippner 1986). From this viewpoint, it has been argued that the quest for repeatability should be abandoned (Pratt 1974).

Despite the arguments about the relative unimportance of repeatability, the history of science demonstrates that unrepeatable experiments or demonstrations should be viewed with a very cautious eye. Most parapsychologists probably would not dispute this point. Indeed, the claim is made that the level of repeatability that has been demonstrated in parapsychology exceeds typical replicability rates in the social sciences; the

longest claim in this regard concerns the psi ganzfeld effect, for which replicability is said to be in the area of 50% (Honorton 1976; 1978b). This research is discussed in the next section.

In summary, then, although one cannot set precise standards that evidence of psi must meet, judgment should be suspended until there is at least some consistency among research findings from a body of methodologically irreproachable experiments, at least some of which are repeatable in Beloff's (1984) strong sense.

### Is there any substantive evidence that psi exists?

Within parapsychology itself, there are arguments about the strength of the evidence adduced for psi. Some argue that no substantive evidence has yet been found (e.g., Barker 1978), whereas others consider the laboratory evidence for psi convincing (e.g., Schmeidler 1984); still others believe that psi can even now be harnessed – for example, to guide stock market investments (Targ & Harary 1984). On the whole, it would appear that most parapsychologists believe that psi has already been demonstrated. Schmeidler (1971) reported that almost 90% of a small sample of members of the Parapsychological Association indicated they believed that ESP had been established so firmly that any further research aimed only at demonstrating its existence would be uninteresting. Subsequently, in a survey of all 241 members and associates of the Parapsychological Association (which yielded a response rate of 84%), 68% indicated complete belief in the reality of psi (McConnell & Clark 1980). The average strength of belief across all respondents was 93%.

Many studies have been carried out and published that purport to provide statistical evidence for paranormal processes. However, even if we were willing to treat certain statistical deviations as evidence of psi, such evidence has been unsatisfactory: A number of recent analyses have demonstrated a serious problem with the quality of the methodology used in parapsychological research. For example, Akers (1984) cites a survey of 214 K experiments (May et al. 1980), in which the authors concluded that none had been properly designed and reported.

In order to explore in more detail the state of the evidence in parapsychology, five major areas of contemporary parapsychological research will be discussed below.

**4.1. Out-of-body experiences.** Blackmore (1982; 1984), after carefully studying both the anecdotal and research literature on out-of-body experiences (experiences in which the individual believes that the physical body has been left behind and that travel through physical space is therefore unencumbered by limitations imposed by the flesh) and after conducting her own research, came to the conclusion that normal psychological theories are capable of accounting for such experiences and that nothing paranormal is likely to be going on.

**4.2. Personality/attitudinal variables and psi.** Akers (1984) evaluated 54 experiments that studied the influence of altered states and of personality/attitudinal variables on psi and that had been cited as significant confirmations of

psi. He found 96-00789R002200120001-4 were seriously flawed, and even the 8 that were conducted with reasonable care were not methodologically ideal. The problems fell into several categories, including randomization failures, sensory leakage, inadequate safeguards against subject cheating, the possibility of errors in the recording of the data, errors in statistical analysis, and failures to report important procedural details. Akers concluded that these 54 experiments taken together were too weak to establish the existence of a paranormal phenomenon.

**4.3. The psi ganzfeld effect.** As mentioned earlier, studies of ESP in a ganzfeld (a condition of reduced sensory stimulation typically produced by covering a subject's eyes with halved Ping-Pong balls and shining a white light onto them while playing white noise into the subject's earphones) have been very promising in that they have appeared to demonstrate a replication rate of 50% or higher (Blackmore 1980; Honorton 1978b).

Hyman (1985b) has completed an exhaustive analysis of virtually all psi ganzfeld research, using a data base of 42 studies conducted between 1974 and 1981. Hyman's analysis leads him to conclude that the replication rate exhibited in this collection of studies is probably very close to what would be expected by chance. Several flaws of procedure – including less than adequate randomization, the possibility of sensory leakage, and erroneous statistical analysis – plagued these studies; not a single study was flawless, he reported. He suspects that most of these studies were not well planned, and he concludes that this data base is too weak to support any assertions about the existence of psi. However, Honorton (1985) disputes Hyman's conclusions, arguing that his assignment of flaws is itself seriously flawed, and he maintains that these studies do indeed indicate a significant psi ganzfeld effect.

Hyman and Honorton (1986) prepared a joint paper as a follow-up to the two papers discussed above. With reference to the data base discussed earlier, they agree that the experiments as a group departed from ideal standards on aspects such as multiple testing, randomization of targets, controlling for sensory leakage, application of statistical tests, and documentation. Although we probably still differ about the extent and seriousness of these departures, we agree that future psi ganzfeld experiments should be conducted in accordance with these ideals. (p. 353)

They go on to say that

whereas we continue to differ over the degree to which the current ganzfeld data base contributes evidence for psi, we agree that the final verdict awaits the outcome of future psi ganzfeld experiments – ones conducted by a broader range of investigators and according to more stringent standards. (pp. 352–53)

Thus, although the ganzfeld studies have been offered as the strongest evidence for a repeatable psi effect, any conclusion about a psi ganzfeld effect must await future research carried out more carefully than these studies were.

**4.4. Remote-viewing studies.** In 1974 *Nature* carried an article by two physicists (Targ & Puthoff 1974) in which they described their successful demonstrations of "remote viewing," a talent by means of which subjects are

able to describe geographical locations being visited by other people without having any normal form of communication with them. This putative skill is said to be within everyone's capability (Targ & Puthoff 1977). For a period of time, this research seemed to promise a breakthrough in the search for a demonstrable psi effect. However, Marks and Kammann (1978; 1980), unable to replicate the remote-viewing effect themselves, discovered serious flaws in the remote-viewing procedure – flaws that they argued accounted for the observed effects.

The principal flaw concerned the judging procedure: Judges were asked to match up a series of responses against a set of targets. Marks and Kammann argue that because the transcripts of the subjects' reports were not edited to remove cues that would assist the judges in identifying the targets, the judging procedure itself – and not any psi effect – produced above-chance matching of transcripts with targets. Tart et al. (1980) responded to this criticism by first having the transcripts edited to remove any possible extraneous cues, and then having them rejudged. They reported that this did not eliminate the remote-viewing effect. However, Marks and Scott (1986), after obtaining access to the relevant findings (they had until recently been denied access to the raw data), report that the editing of the transcripts had failed to eliminate all the extraneous cues and that enough cues remained to account for the above-chance scoring rate.

There have been other criticisms of the remote-viewing studies as well, including concerns about statistical problems that could give rise to above-chance scoring rates (Hyman 1977b), and about the lack of adequate controls and control groups (Caulkins 1980). A number of replications and extensions have been reported (e.g., Bisaha & Dunne 1979; Dunne & Bisaha 1979; Schlitz & Gruber 1981; Schlitz & Haight 1984); only the Schlitz and Haight (1984) study appears to avoid the weaknesses of the Targ–Puthoff series, but even here, there was no control condition to allow proper assessment of the background "coincidental" scoring rate.

Thus, the Targ–Puthoff series is too flawed to be of evidential value, and none of the subsequent published studies have been carefully enough controlled to bear testimony about psi.

**4.5. Schmidt's random-event generator (REG) studies.** For almost 20 years, Helmut Schmidt has been conducting research into the ability of subjects to predict or influence the radioactive emission of subatomic particles. His research enjoys generally high regard from other parapsychologists: Beloff (1980), for example, views some of Schmidt's research as being among the most evidential in all of parapsychology, despite his own inability to replicate Schmidt's findings.

Schmidt has published a considerable number of studies. Unfortunately, this investigator typically completes a study and then – rather than focusing on a given research question, or refining his measurements, or examining the effects of various parameters in that particular situation, or working with one type of generator over a period of time so that he and others can come to appreciate its idiosyncrasies – he moves on to a totally different situation altogether (Hansel 1980), changing the design and components of his generator as he goes along (Hyman 1981). This makes it very difficult for him or anyone

reading his research reports to learn the limitations of his generator or his procedures.

Little of Schmidt's research is free from serious methodological shortcomings (Hansel 1980; 1981; Hyman 1981). Consider, for example, one of his initial studies (Schmidt 1969b), which has been favorably cited many times in the parapsychological literature. The situation was as follows: A subject was seated before a panel of four lights and four corresponding buttons. On each trial, the subject would press one of the buttons to predict which light would next illuminate, something that would be determined by particle emission from a strontium-90 source. The light would then illuminate, giving immediate feedback. If the light corresponded to the depressed button, it was a "hit."

In the first experiment in this report, Schmidt combined the results from his three subjects and obtained a hit rate significantly higher than would be expected by chance: 0.261 as compared to 0.250 ( $p < 2 \times 10^{-9}$ ). In the second experiment, subjects were allowed to choose to try to make a high or a low number of hits. Here, the combined scoring rate of three subjects was 27%, again significantly higher than chance expectation ( $p < 10^{-10}$ ).

Both experiments suffered from less than optimal experimental control; as in most of Schmidt's studies, subjects were usually unsupervised, and there was a general lack of rigour in the control of experimental conditions. Hansel (1980) objected to the fact that the exact numbers and types of trials undertaken by each subject were not specified in advance, and also to the fact that the equipment, although partially automated, did not rule out cheating during data classification.

There is a more fundamental concern about these experiments: the target series (Hyman 1981). Schmidt compared the subjects' hit rates to chance expectation, but this assumed that the target series was random. (Particle emission is presumably random; the output of his generator was not necessarily so.) Schmidt's randomization checks were carried out on target strings much longer than those used in the experiments, and therefore did not allow the detection of possible short-term biases in the generator which could give rise to nonrandom target strings. Because immediate feedback was provided throughout the experiment, and because subjects were free to "play" with the equipment and to decide when to start and stop a given session, any undetected short-term bias in the generator might give the subject the impression of being "hot" and therefore lead him to initiate a session, which he would probably end once he seemed to turn "cold." This, of course, could produce above-chance scoring rates.

It would therefore be important and appropriate to analyze the *actual* target sequence in terms of how well it conformed to what would be expected by chance. However, were one to find that the target sequence was nonrandom, this could, after the fact, be taken as evidence of PK. Indeed, Schmidt reported that after the testing one subject said he had tried to affect the outcome rather than just predict it; he had tried to produce more illuminations of lamp no. 4, he said. It was found for this subject that there was indeed an excess of 4s in his target series. No indication is given in the report as to whether this analysis of targets was carried out for other subjects, and if not, why not. However, Schmidt subsequently

3. In a related vein, Schmeidler (1984) reports that PK effects are often strongest just *after* a session has terminated or during a subject's rest period. Rather than ignoring data accumulated after the session is over, this is taken to reflect another psi phenomenon, and has been given two names – the "linger effect" and the "release of effort effect." If this is to be taken seriously, then all researchers should report not only the presence of such an effect, but its absence as well: were this done, the frequency of the effect may well turn out to be within the bounds of normal statistical expectation.

4. Some parapsychologists seem consistently to obtain the results they desire whereas others are unable to find significant departures from chance (Palmer 1985b). The failure of one researcher to obtain significant results using the same procedure that yielded significant results for another researcher, rather than being taken as a failure to replicate or as a hint that extraneous variables may be producing artifactual results, is often interpreted in terms of the *experimenter effect*. This effect is so common in psi research that it has even been described by one parapsychologist as parapsychology's one and only finding (Parker 1978). To *describe* the fact that two researchers obtained different results by calling it an experimenter effect is quite appropriate. After all, the experimenter effect as such is by no means unique to parapsychology, and a great deal has been written on the subject with regard to research in psychology and other domains (see Rosenthal & Rubin 1978). However, in psi research the term is all too often used more as an *explanation* than as a description, and that is because it is considered that the effect may result not only from experimenter error (in that one experimenter may be more successful in obtaining psi effects than another because he unwittingly allows more artifacts to contaminate his procedure), or from differences in personalities (in that some experimenters may put their subjects into a more comfortable and psi-conducive frame of mind than others), but also from the psi influence of the experimenter himself (Krippner 1978a; Palmer 1985b; 1986b). If psi exists, of course, it would only make sense that the experimenter, who naturally wants his experiment to succeed, might unknowingly bring his psi influence to bear, whereas a skeptical or neutral experimenter might not use psi at all, or might use it to prevent the appearance of a subject psi effect. This whole problem leads Palmer (1985b) to describe the experimenter effect as the most important challenge facing parapsychology today. It is hard to imagine scientific inquiry of any sort if the results of the investigation are determined by the psychic influence of the investigator (Alcock 1985; see also Krippner 1978b).

The experimenter effect (or the experimenter psi version of it) provides a powerful method for undermining failures to replicate, and is sometimes resorted to for just that purpose. For example, when Blackmore (1985), a devoted parapsychologist for many years, found herself becoming increasingly skeptical about psi as a consequence of her inability to produce experimental evidence for it, she noted that "many parapsychologists suggested

this same piece of apparatus in a PK experiment (Schmidt & Pantazis 1972) in which the only task was to try to influence the machine to produce an excess of 4s above-chance scoring rates were reported in that instance as well, which led Schmidt again to conclude that was operating. The skeptic is left wondering whether the apparatus simply produces an excess of 4s from time to time. Certainly, nothing can be concluded from such efforts until more is known about the target series produced by the generator.

Thus, a study that seems at first to offer considerable evidence of an anomalous process is found to be badly flawed. It would make sense for Schmidt to redo the study, taking steps to make these criticisms unnecessary. Generally lacking in Schmidt's studies is a proper control condition: One should generate *pairs* of runs, with one designated, on the basis of some random procedure such as the toss of a coin, as the experimental and the other as the control for each trial (Hansel 1981).

The problems in this study recur over and over in Schmidt's research (Hansel 1980; 1981; Hyman 1981). Only one of his studies appears well designed (Schmidt et al. 1986). However, we must wait to see whether the psi effect apparently obtained in this very recent study stands up to replication. There have been many psi studies (e.g., Bierman & Puthoff 1974) in the past that at first appeared beyond reproach, only to be found later to be seriously flawed.

In summary, these various areas of research are plagued by methodological and statistical flaws of one sort or another. Until research is undertaken that is methodologically well planned and well executed – as Hyman and Honorton (1986) recommend with regard to the ganzfeld – there is little point in debating whether or not the existing evidence establishes a case for psi.

## Does parapsychology follow the rules of science?

Of course, by using the term "rules of science," one could open up all manner of dispute because of the difficulty that exists in listing those rules or in demarcating science from pseudoscience (e.g., see Bunge 1984; Edge & Morris 1986). Rather than tackle that conundrum, it is more profitable to examine several aspects of parapsychological endeavor that appear to run counter to the spirit of scientific inquiry; each is discussed below:

**5.1. Unfalsifiability.** There are a number of principles in parapsychology that can be used to explain away failures to find empirical support for a hypothesis, thus creating a situation of unfalsifiability:

1. Perhaps the subject did significantly worse than expected by chance. If so, this may be taken as evidence of psi, because it seems to be *psi-missing*, something which occurs so often that it is now taken to be a manifestation of psi (e.g., Crandall & Hite 1983).

2. If outstanding subjects subsequently lose their psi ability, or if subjects do more poorly toward the end of a session or of a series of trials, this is labeled the *decline effect* (e.g., see Beloff 1982). Rather than being taken as a possible consequence of either statistical regression or the tightening up of controls (when that has occurred), the decline effect often takes on the power of an explanation.

that the reason I didn't get results was quite simple - *me*. Perhaps I did not sufficiently believe in the possibility of psi" (p. 428).

In summary, it is the way such "effects" are used - and not, in principle, the research procedures - that vitiates the scientific respectability of parapsychology, for they make the psi hypothesis unfalsifiable by providing ways to explain away null results and nonreplications. These descriptive terms have mistakenly come to be taken as properties of psi, which leads to the circularity of explaining an observation by means of the label given to it. Moreover, as important properties of psi, their *nonappearance* in a psi experiment should weigh against any conclusion that psi has occurred; this never happens in the parapsychological literature.

**5.2. All things are possible.** Another aspect of parapsychology that makes critics uncomfortable is what seems to be almost an "anything goes" attitude, with no speculation seeming too wild. For example, so-called observational theory based on parophysical interpretations of quantum mechanics, predicts that random events can be affected simply by being observed, even if the observation occurs at some time in the future (see Bierman & Weiner 1980). In line with this notion, studies have been done which claim to show that subjects can exercise an influence backwards in time ("retroactive PK") so as to affect the choice of stimulus materials preselected for the study in which they are participating (e.g., Schmidt 1976). This also means, of course, that the present is possibly being influenced by future events (Martin 1983). A "checker effect" has also been postulated, in which ESP scores may be retroactively and psychokinetically influenced by the individual who checks or analyzes the data (Palmer 1978; Weiner & Zingrone 1986). Schmidt (1970c) reported that cockroaches were able to influence a random-event generator in such a way as to cause them to be shocked *more* often than would be expected by chance. He suggested that perhaps his own psi, fueled by his dislike of cockroaches, accounted for the increase, rather than a decrease, in shocks.

Not only can psi apparently transcend temporal boundaries, it also seems that no effort, no training, and no particular knowledge are required to use it. Indeed, modern PK studies appear to indicate that psi is an *unconscious* process, but a goal-oriented one in that it helps the individual attain desired objectives: Success in a PK experiment does not require knowing anything about the target, or even knowing that one is in a PK study (Stanford 1977). Thus, psi appears to operate very much like wishful thinking. For example, going back to the Schmidt (1969b) study, all that was needed, it seems, was for that one subject to *wish* for a particular light to come on and it would light up statistically more frequently than the others. (Of course, when subjects do score above chance, neither they nor anyone else can say which hits were brought about by psi and which were the consequence of chance.)

As I have argued earlier (Alcock 1984), the fact that no physical variable has ever been shown to influence the scoring rate in psi experiments (Rush 1986c), combined with the apparent total lack of constraints on the conditions under which psi can be manifested (whether forward in time, backward in time, across thousands of

miles, between humans and objects, between humans and animals, or even between animals and objects), serves to weaken the a priori likelihood that psi, as any sort of force or ability, exists. After all, most psi experiments are very similar, in that all that is typically done is to examine two sets of numbers, representing targets and responses in an ESP experiment or outcomes and aims in a PK experiment, for evidence of a nonchance association. It may simply be that the enterprise of parapsychology generates, from time to time, significant statistical deviations - be they the result of artifact, selective reporting, or whatever - which are then independent of the research hypothesis, so that no matter what the researcher is examining - the effects of healing on fungus, PK with cockroaches, ESP across a continent, or retroactive psi effects - the likelihood of obtaining significant deviations remains the same. (For example, if an REG produces an excess of 4s on a short-term basis, and if the procedure allows subjects to tap into this, then it should make no difference in principle whether the targets are generated on-line or were recorded a week earlier: If the subject aims for more 4s, he will obtain them.) Difficulty in replication by other researchers using their own equipment or slightly different procedures would, of course, follow from such a state of affairs, as would the experimenter effect.

This psi-as-artifact notion is not offered as an empirically testable hypothesis. I only mean to show that the lack of constraints on the appearance of psi undermines rather than strengthens its credibility. It would be hard enough to accept that a philosopher's stone can turn base metals into gold, as alchemists believed. It would be harder still to believe that it can turn *anything* into gold and that anyone can use it without any training.

**5.3. Lack of rapport with other areas of science.** Parapsychology, despite its efforts to find common areas of interest with other research fields (see the *Handbook of Parapsychology* [Wolman 1977a]), has failed to establish any genuine overlap with other disciplines, because, so far at least, other disciplines do not seem to *need* psi. If "normal" explanations for strange physical or psychological phenomena were exhausted, and/or if the influence of the researcher's consciousness appeared to have an effect on the way matter behaved in "normal" experiments, then a much greater number of scientists might be more open to the possibility of psi. Indeed, if parapsychologists are right about psi, then the well-tested theories of physicists and neurologists are wrong (Hebb 1978). It is perhaps noteworthy that the claims that psi can influence radioactive decay do not come from particle physicists in the course of their everyday work.

## 6. Are the critics fair?

Some parapsychological proponents, such as Child (1985), argue that few in "normal" science bother to immerse themselves in the details of parapsychology, and instead gain a false or misleading impression from the accounts given by their colleagues who serve as critics of the field. Such critics are accused of unfair tactics, such as (a) arguing that unless fraud can be ruled out, it is the most parsimonious explanation of psi claims; (b) setting higher standards for parapsychological research than for

in the realm of normal science; and (c) simply fitting the possibility of psi out-of-hand (see Collins & Pinch 1979).

Charles Tart (1982; 1984), a former president of the Psychological Association, suggests that there is anational basis for critics' unwillingness to welcome psychology into the scientific fold, an argument that has been repeated by Schmeidler (1985) and Irwin (1985b), among others. Tart posits that a widespread and unconscious fear of psi has developed either because psi ability would disrupt social functioning (because we would have access to one another's true feelings and thoughts) or because of what he calls "primal conflict resolution": A mother often feels angry toward her child, keeps her cool and speaks to the child in a positive, corrective way. The child, if psi is already operating, is faced with a frightening conflict of messages and learns to suppress psi altogether so as to avoid the information conflict creating such conflict. Targ and Harary (1984), on the other hand, argue that skeptics base their opposition on rationality but on religious conviction.

Suggestions about fear and religious conviction are too weak and *ad hoc* to require rebuttal. Collins & Pinch's (1979) concerns, on the other hand, are important. However, they could be equally relevant to any controversial claim, and thus nothing abnormal seems to be going on in the critical reactions to parapsychology. The scientific arena is a tough one; many ideas march in to do battle; some survive, but just as many perish. Numerous other controversial claims have faced hostility and even derision from scientists; some of these have won out (e.g., continental drift – see Hallam 1975); others (e.g., polychromer – see Franks 1981) have not. Psychologists were at first unwilling to believe in the notion of biological preparedness with regard to learning (i.e., the idea that organisms, including humans, are biologically prepared to learn certain kinds of aversions more rapidly than others), and the leading journals refused to publish research reports on the subject, reports that are now viewed as being among the most important in their field (Ligman & Hager 1972). This concept is now part of mainstream psychology. Many psychologists also refused to believe in biological constraints on intelligence (or, at first, racially determined ones); and as a result of such a refusal to believe, the fundamental studies in this area – reported by Sir Cyril Burt – were eventually exposed as fraudulent (Kamin 1974). When, in the late 1960s, Neal Miller announced that he and L. Dicara had demonstrated operant conditioning of heart rate in rats (Miller & Dicara 1967), many experimental psychologists refused to believe it, despite Miller's high reputation as an experimental psychologist. Ultimately, Miller himself, when subsequently unable to replicate his own studies, publicly withdrew his claims (e.g., Miller 1978). Science refused to publish, on the grounds that it was erroneous, the initial research of Solomon Berson and Rosalyn Yalow (Yalow subsequently won the Nobel Prize) in the insulin-binding antibody, research that was fundamental to the development of the radioimmunoassay technique (Garfield 1986; Yalow 1978). Albert Einstein absolutely refused to believe that "God plays dice," despite the implications of quantum mechanics; he chose to believe the theory to be in error due to incompleteness. Science is full

This process, although sometimes seemingly cruel and dogmatic, is perhaps necessary to allow scientists to focus on claims that appear most promising, rather than being distracted by others that appear to have little to recommend them. Sooner or later in science, it seems, the truth will out, and error falls by the wayside. Even acupuncture, long regarded as being nothing short of superstition, is now regarded as capable of producing limited pain relief (Zusne & Jones 1982).

If the insulin-binding antibody, biological preparedness, and acupuncture analgesia won accommodation in science, it is because the evidence for them became so strong that they *had* to be accommodated. A century of parapsychological research has gone by, and the evidence for psi is no more convincing now than it was a century ago.

It seems accordingly that parapsychologists who attack scientists and critics for their refusal to recognize the importance of psi and of psi research are attacking the messengers because they cannot accept the messages they bear. Suppose that instead of psi, parapsychologists were promoting a cure for baldness, but that the amount of hair produced by the treatment was tiny and detectable only by some researchers, sometimes. If the effect is unreliable and unrepeatable, if it also contradicts all that is known about hair growth and alopecia, and if there is no theoretical mechanism put forth for the putative effect, then one would hardly expect the scientific community to cheer the end of baldness. Science will never take parapsychologists simply at their word; they must offer a clear, replicable demonstration of a basic phenomenon in order to gain acceptance in science.

Moreover, one can seriously challenge the claim that practitioners of normal science do not give, or have not given, parapsychology its day in court. As was mentioned at the outset, a number of professional scientific organizations have invited parapsychologists to address them or have set up symposia on the subject. True, parapsychological ideas have hardly been embraced with open arms, but that does not mean that scientists are motivated by fear or blind prejudice or ignorance or distorted interpretations purveyed by unreliable skeptics.

Indeed, when parapsychology began to take shape as a serious research field, a good number of psychologists and others immediately took up the challenge of investigating claims of spiritualistic communication, telepathy, clairvoyance, and so on. All that was lacking to make parapsychology part of mainstream psychology was evidence that there was a phenomenon to investigate. At the Fourth International Congress of Psychology, held in Paris in 1900, an entire section was devoted to psychical research and spiritualism, and the president, Ribot, announced the founding of a psychical research institute in Paris (L'Institut Général Psychique) (McGuire 1984). Membership in this institute included a number of prominent psychologists such as Janet, Richet, James, and Tarde. In 1895 Binet published some case studies of telepathy. However, as McGuire (1984) points out, psychologists were already becoming very uneasy about the growing link between psychical research and spiritualism; this mistrust began to show itself at the Fourth Congress, and subsequently many French psychologists began to turn their backs on psychical research.

Psychologists Pieron, Janet, and Dumas conducted a number of seances in which they reexamined mediums who had produced positive outcomes in earlier studies at the Institut Métapsychique. One medium was caught flagrantly cheating, and these psychologists concluded that no psychical phenomena had been observed under the carefully controlled conditions. LeBon offered a large reward to anyone who could produce the mediumistic effects in his laboratory, but once informed of the stringent controls, no one ever underwent the test (McGuire 1984).

The American Society for Psychical Research was set up in 1885 to examine apparent psychical phenomena (Moore 1977). Its officers included prominent psychologists such as Prince, Hall, Jastrow (later to become an outspoken critic), and James. When they failed to find any evidential basis for mediumistic claims, most members lost interest; the group was disbanded, and its remnants merged with the British Society for Psychical Research (SPR). James continued to support and believe in psychical research, and later became president of the SPR.

In the 1930s, parapsychology had another opportunity to persuade mainstream science about the importance of psi research. A poll conducted in 1938 found that 89% of psychologists felt the study of ESP was a legitimate scientific enterprise and 79% felt such research was a proper subject for psychologists (Moore 1977). In that same year, a round-table discussion of parapsychology was sponsored by the American Psychological Association. Parapsychologists did not succeed in their attempts to gain the psychologists' support for the study of psi.

The 1970s provided another period when mainstream science seemed ready to give parapsychology a chance. As mentioned at the beginning of this article, the Parapsychology Association had gained affiliation with the American Association for the Advancement of Science in 1969. In 1974 one of the world's leading scientific journals, *Nature*, published an article by parapsychologists Targ and Puthoff in which they detailed their claims about scientific evidence for the paranormal, based largely on research with Uri Geller (Targ & Puthoff 1974); true, the journal did precede the article with an editorial disclaimer, but the research nonetheless appeared. Although some parapsychologists were irked by the editorial "inoculation" *Nature* provided for its readers, such a disclaimer proved to have been prudent, because, as discussed earlier, Uri Geller was subsequently exposed as a fraud (e.g., Randi 1975).

Although mainstream psychological journals continue to be reluctant to publish parapsychological research, that is not to say that these journals are totally closed to parapsychologists; occasionally articles do appear (e.g., Layton & Turnbull 1975). *American Psychologist* recently published an article (Child 1985) that presented, along with his criticisms of skeptics' interpretations of parapsychological research, the results of a meta-analysis of the classic Maimonides dream studies. Child concluded that something important is going on, although, in my view, his analysis is unlikely to impress many psychologists. Parapsychology was discussed in an open-minded fashion, albeit very briefly, in a recent issue of the *Annual Review of Psychology* (Tyler 1981). Since 1950, more than 1,500 parapsychological papers have been published in the field, and the number continues to grow.

*Psychological Abstracts*, which is published by the American Psychological Association (McConnell 1983).

What more should parapsychologists expect, given the track record they have produced? I am of the strong opinion that rejection of, or dissatisfaction with, paranormal claims is not based on narrow, dogmatic prejudice, but on the fact that after a century of research, there is still nothing substantive to show!

## 7. Is rapprochement between psychology and parapsychology possible?

In 1982 psychologists Zusne and Jones's *Anomalistic Psychology* was published. This book constituted a milestone in the course of interaction between psychology and parapsychology by virtue of its attempts to establish a framework for the psychological study of the phenomena taken by parapsychologists to be paranormal. Blackmore (1983a), coming from the parapsychological side, and just as she was renouncing her belief in the psi hypothesis, also called for the study – within psychology – of the experiences that appear to people to be paranormal. Palmer (1986a) calls for a collective focus by skeptics and parapsychologists on finding explanations for anomalous experiences and phenomena, whether the explanations prove to be mundane or not. These actions may reflect what Truzzi (1985) views as a movement toward rapprochement between psychology and parapsychology.

Unfortunately, I doubt that such a rapprochement will ever occur, for I believe that those in parapsychology who move closer to the skeptical side will fail to draw the rest of parapsychology along with them. That is not to say that there will not be cooperation between psychologists and parapsychologists in the study of anomalistic experiences, something which should be strongly encouraged; nor is it to deny whatever movement there has been toward better mutual understanding and respect.

However, finding explanations for ostensible anomalies is not what parapsychology is really about for most parapsychologists. If it were, much more effort would be made to try to find psychological and neuropsychological explanations for such experiences before even contemplating the radical psi hypothesis. (Indeed, one must wonder why parapsychologists seem not to concern themselves with the actual *experience*, or with *how* such experiences are generated, or with *how* the supposed phenomena work [Scott 1985]. Why, for example, do they not set out to try to produce in subjects the subjective impression of telepathy, instead of merely concluding that subjects in a guessing task must have experienced telepathy on some of the trials? Studying guess rates is not the study of the telepathic *experience*.)

If parapsychology is not primarily motivated to explore anomalies in an open-minded fashion, what is its motivation? Why does parapsychology persist after a century of failing to produce compelling evidence of psi? Why does the psi hypothesis survive? To be fair, of course, normal science does not reject working hypotheses just because they fail to be confirmed empirically – although they rarely, if ever, show such longevity. For example,

the existence of capillaries, and such capillaries not be observed with the naked eye; but the failure serve them did not lead to rejection of the theory. Investigators continued to seek them until, with the aid of scopes, they were at last discovered (Gregory 1982).

Yet, there is a difference between, on the one hand, not giving up a preferred hypothesis when that hypothesis seems to promise more explanatory power than existing theories about a range of observations and, on the other hand, the discounting of failure to find tested statistical deviations in a psi experiment. In the first case, one is trying to establish the existence of a phenomenon that is *not* required by the existing body of scientific data, nor is it predicted by theory, nor would it clarify or clear up current anomalies in physics or biology or biology.

The dispute about psi reflects the clash of two fundamentally different views of reality. The first of these is the materialistic, monistic view that the human mind is some kind of emergent manifestation of brain processes, whereas the second is the dualistic position that maintains that human mind/personality is something beyond the sum of atoms and molecules. Parapsychology grew out of the second of these; it developed directly from attempts, first in Europe and the United States, to put the post-death survival of the human personality on a sound scientific footing (Cerullo 1982; Mauskopf & McVaugh 1970; Moore 1977). It is the search for the soul – not the soul as it is described by various religions, and perhaps not even the secularized soul sought by the psychical researchers of the late nineteenth century during the heyday of spiritualism (Cerullo 1982), but a soul all the same. Because, if the mind can operate separately from the physical brain, as the psi hypothesis would suggest, then it possesses much of what has been ascribed to the soul.

Most religions teach that the Soul survives death in some form. The question of survival of the parapsychologists' "soul" or "mind" or "personality" after death is, in many leading parapsychologists agree, an important question for parapsychology to consider (e.g., Krippner 1983; Palmer 1983; Roll 1982). Blackmore (1983b) suspects that just as it was the *fundamental* question to many of the early psychical researchers, it is still so for many of the fellow researchers today.

Thus, it is important in any debate about parapsychology to make clear just what is being debated. Is the debate about whether or not there exist "natural" phenomena that science has so far failed to recognize, or is the debate about whether or not dualism, as opposed to materialistic monism, is the correct view of nature and of mankind's place in nature? Or, is the first question very often the surface issue, while the hidden agenda is the question of dualism?

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## Conclusion

Either parapsychology is a harvest of false illusion, or the meat and fibre of biology, the focus of psychology, and even the material conception of physics on which all science stands" (Walker 1984, p. 9). These words by a parapsychologist should remind us that the existence of psi is

no trivial matter. Yet, to accept the reality of psi, we must accept that some force or process exists which cannot at this time be described in terms of positive properties, but only in terms of what it is not; a force which is capable of allowing for direct communication between two brains, regardless of the distance between them; and which allows the mind directly and often unconsciously to influence matter in such a way as to gain some desired goal, again without any effect of distance, physical barriers, or even time. To accept the reality of psi, we must discount a hundred years of failure to find substantive evidence; there is not a single demonstration that is repeatable in Beloff's "strong sense." We must also accept that there are fundamental problems with well-tested physical and neurophysiological theories. We must accept all this in the face of the inability of parapsychologists to sort out whether, in a given experiment, a statistical deviation is due to PK or to ESP, whether it is due to the subject or to the experimenter, and whether the source of psi is acting in the past, the present, or the future. Furthermore, we must overlook the fact that even the best research programs in parapsychology are seriously beset by methodological weaknesses. We must ignore history as well, for as Hyman (1981) points out, each generation of parapsychologists has put forth its current candidates as providers of proof of psi – experiments that supposedly should have convinced any rational person were he to examine the evidence fairly. Yet, these candidates keep changing, and if prior history is a reliable guide, today's most promising research programs in parapsychology may well be passé in a generation or two.

If parapsychologists really are dedicated to the study of anomalous experience, then it should make more sense to follow Blackmore's (1983a) lead and focus on the anomalies while putting the concept of psi aside until, if ever, it is needed. This is unlikely to happen, however. Psi has been postulated not because normal psychology is incapable of accounting for people's apparently psychic experiences, nor because of inexplicable findings in physics or chemistry; nor is it the logical outgrowth of some compelling scientific theory. Rather, the search for psi is now, as it has been since the formal beginning of empirical parapsychology over a century ago, the quest to establish the reality of a nonmaterial aspect of human existence – some form of secularized soul.

All that is needed to turn the attitude of the scientific establishment from doubt to serious interest with regard to psi is to produce some clear, substantive evidence of a psychic phenomenon. Without it, parapsychology can never become a science.

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